PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

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At the first ordinary meeting of the Society on 7th December, the Makdougall-Brisbane Prize, for the Biennial Period 1866–68, was awarded to Dr Alexander Crum Brown and Dr Thomas Richard Fraser, for their paper on the Connection between Chemical Constitution and Physiological Action.

At the same meeting the Neill Prize, for the Triennial Period 1865–68, was awarded to Dr William Carmichael M'Intosh, for his paper on the British Nemerteans and on some new British Annelids.

Monday, 4th January 1869.

Dr LYON PLAYFAIR, C.B., M.P., Vice-President, in the Chair, who said—

It was my painful duty last year to allude to the death of that great philosopher, Sir David Brewster, and within a few months we have now to deplore the loss of another philosopher, also great —I need not say that I allude to Principal Forbes. This Society is intimately identified with his life and labours. Long our Secretary, he did all that was in his power to promote its success. As a man of science, we are too intimate in this place with his researches to render it necessary that, on the present occasion, I should make a detailed allusion to them. His early and his latest researches were upon heat. He established the polarisation and double refraction of the heat ray, and proved the identity of thermal and luminous radiations. His last published research is upon the conduction of heat by iron. In this he has established that, like electricity, heat passes more slowly through a bar of elevated

temperature than through one which is cold. He also showed how the absolute conductivity of a metal for heat might be measured.

But it is probably in connection with his researches on glaciers that Forbes's name is best known to the general public. His first Memoir on the veined or ribboned structure of Ice was published in our Transactions in December 1841. Reading that memoir with the light of recent researches, we rise profoundly impressed with the accurate observations and clearness of judgment of their author. As he himself observes in that paper, it is astonishing how little we see until we are taught how to observe. This veined structure of glaciers is intimately connected with their mode of formation, and with the remarkable phenomena which render them so interesting to all investigators; and yet no one observed with the eyes of science this important veined structure until Forbes described it. Even with the new experiments of James Thomson on the lowering of the freezing point of ice under pressure, and of those of his brother, Sir William Thomson, on the rupture produced in a viscous solid by continued shearing, we could scarcely at the present day observe the phenomena more accurately than was done by Forbes twenty-nine years ago, or connect them more lucidly with the occurrence and position of the cracks and crevasses of the glacier.

But this and all his subsequent researches on the motions of glaciers as a viscous mass exhibit the peculiar characteristic of Forbes's mind—scrupulous conscientiousness in his scientific labours, scrupulous conscientiousness in his life as a man.

It is not my duty to say more than I have done; but at the first meeting of the Society which has occurred after his death, I thought it right to allude to our own loss; and I now move that the Society instruct the Council to express to Mrs Forbes and her family our sympathies for their bereavement, and our sense of the loss which science has sustained by the death of this distinguished philosopher.

Professor Jenkin, at the request of the Council, delivered an Address on Cable Testing.

The following Communications were read :—

1. Notice of a Heart in which the Superior Vena Cava possessed a Valve at its Auricular Orifice. By Professor Turner.

Whilst examining the heart in the body of one of the male subjects undergoing dissection in my practical rooms, in the early part of this session, the large size and fenestrated condition of the Eustachian valve attracted my attention. When the heart was removed from the body, and the auricle submitted to a more careful scrutiny, I observed that a valve was situated at the mouth of the superior vena cava.

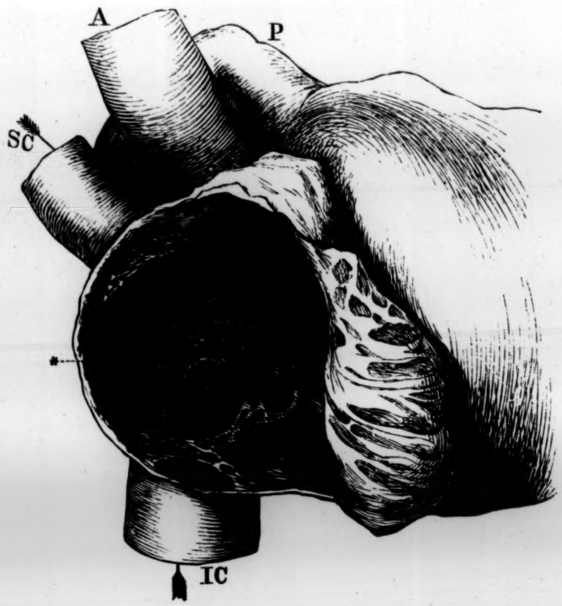
As I have not, up to this present time, seen any record in the various anatomical works which I have consulted of the existence of a valve in this locality, I am induced to bring the specimen before the notice of the Society.

A membranous valve, formed by a reduplication of the endocardial lining membrane, lay across the anterior and inner border of the auricular orifice of the superior vena cava, and hung pendulous in the auricular cavity. It measured $1\frac{1}{8}$ inch in its long or transverse diameter, but was scarcely $\frac{1}{2}$ an inch deep; so that when drawn across the mouth of the vein, it did not cover over much more than one third of the orifice. Its free border was almost straight. The attached border was semilunar in form, and connected to the wall of the auricle, close to its line of junction with the anterior wall of the vein, not by a continuous membrane, but by numerous slender fibrous bands. Between these bands were apertures of various sizes, one of which, larger than the rest, was situated at the upper and inner part of the valve, which consequently had a fenestrated appearance. From the outer (right) end of the valve, and continuous with its free border, a fibrous cord (*) arched downwards beneath the lining membrane of the right wall of the auricle, and became continuous with the right border of the Eustachian valve.* The inner (left) end of the valve was connected to a short papillary muscle, which was continuous with the muscular wall of

* Since the specimen was shown to the Society I have recognised in two hearts, which have come under my notice in my dissecting room, a similar fibrous cord, extending from the Eustachian valve along the right posterior

the auricle. There did not appear to be any deficiency in the thickness of the muscular coat of the superior cava.

The Eustachian valve projected for upwards of an inch into the auricular cavity, and presented in a remarkable degree the fenestrated character which that valve occasionally exhibits in the adult heart. At its left extremity it subdivided into two parts, one of which passed in the usual way to the auricular septum, and became continuous with the annulus ovalis, whilst the other was blended with the valve at the mouth of the coronary sinus, which also exhibited a fenestrated appearance.



A. Aorta. P. Pulmonary artery. SC. Superior, and IC. Inferior cava. * The fibrous cord connecting the two valves. C Coronary sinus.—*From a drawing of the specimen by Mr T. D. Nicholson.*

Owing to its fenestrated condition, and small size, when compared with the orifice of the superior cava, it is obvious that the valve situated at the mouth of that great vein could have had but little influence in preventing the regurgitation of blood during the contraction of the auricle in the adult heart. But it is probable that in the fœtal stage of this heart the backward flow

wall of the auricle, almost as far as the orifice of the superior cava, but no valve existed at the mouth of the superior cava.

into the vein would have been very considerably impeded by its presence; for there is reason to believe that the fenestrated state, not only of this, but of the other valves at the mouths of the great veins, is due to atrophy taking place after birth. As the muscular coat of the superior cava possessed its usual thickness, the valve was obviously not developed to compensate for any deficiency in that portion of the wall of the great vein.

As the Eustachian valve at the mouth of the inferior cava serves in the fœtus to direct the current of blood passing upwards along that vein through the foramen ovale, it is possible that the valve at the mouth of the superior cava may have exercised some directing effect on the blood which entered the auricle by the latter vessel. From its position it would, I think, have directed the blood of the superior cava away from the auricular septum, and thus have aided in preventing, during the fœtal condition, the mingling of the blood of the two cavæ in the auricular cavity.

The occurrence of such a valve is not, however, of interest merely in its physiological relations: it possesses also a morphological value. For it may be regarded as presenting in the human heart a rudimentary example of an arrangement which is met with in the heart of the bird. If the heart of a large bird, *e.g.*, the ostrich (*Struthio camelus*), be examined, it may be seen that the sinus, into which the venæ cavæ open, is separated from the auricle proper by a large double muscular valve. The right segment of this valve is related not only to the mouth of the right superior cava, but extends down the wall of the auricle to the mouth of the inferior cava, and is then prolonged as far as the mouth of the left superior cava, which may be regarded as representing in position the coronary vein in the human heart. Now, in this specimen it will be remembered that the valve at the mouth of the superior cava was continued, through the intermediation of a fibrous cord (*), into the Eustachian valve, and that the latter again was directly united with the valve at the mouth of the coronary sinus.

As additional illustrations of the tendency to the development of rudimental structures in this individual, it is of interest also to mention that he had in each upper arm a *processus supra-condyloideus humeri internus*, the relations of which to the supra-condyloid foramen of various of the mammalia, more especially of the

carnivora, was described in 1839 by A. W. Otto, and in 1840 and 1841 by Robert Knox, and many examples of which have since that time been recorded by other anatomists. From the tip of this process a band of fascia passed down to the inner condyle, so as to complete the boundary of the foramen, through which the median nerve and the ulnar artery passed; for in both upper arms a high division of the brachial artery had taken place; and whilst the radial branch closely followed the inner border of the biceps, the ulnar was deflected from its course, and passed, along with the median nerve, behind the supra-condyloid process.

2. On the Motion of a Pendulum affected by the Rotation of the Earth and other Disturbing Causes. By Professor Tait.

(*Abstract.*)

1. Let α be the vector (from the earth's centre) of the point of suspension, λ its inclination to the plane of the equator, a the earth's radius drawn to that point; and let the unit vectors i, j, k be fixed in space, so that i is parallel to the earth's axis of rotation; then, if ω be the angular velocity of that rotation

$$\alpha = a\left[i \sin\lambda + (j\cos\omega t + k\sin\omega t)\cos\lambda\right] \quad . \quad . \quad (1).$$

This gives

$$\dot{\alpha} = a\omega(-j\sin\omega t + k\cos\omega t)\cos\lambda$$
$$= \omega\,\mathrm{V}i\alpha \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2).$$

Similarly

$$\ddot{\alpha} = \omega\,\mathrm{V}i\dot{\alpha} = -\omega^2(\alpha - ai\sin\lambda) \quad . \quad . \quad . \quad . \quad (3).$$

2. Let ρ be the vector of the bob m referred to the point of suspension, R the tension of the string, then if α_1 be the direction of pure gravity

$$m(\ddot{\alpha} + \ddot{\rho}) = -mg\,\mathrm{U}\alpha_1 - \mathrm{RU}\rho \quad . \quad . \quad . \quad . \quad (4),$$

which may be written

$$\mathrm{V}\rho\ddot{\alpha} + \mathrm{V}\rho\ddot{\rho} = \frac{g}{\mathrm{T}\alpha_1}\mathrm{V}\alpha_1\rho \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5).$$

To this must be added, since r (the length of the string) is constant,

$$\mathrm{T}\rho = r \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6),$$

and the equations of motion are complete.

3. These two equations (5) and (6) contain every possible case of the motion, from the most infinitesimal oscillations to the most rapid rotation about the point of suspension, so that it is necessary to adapt different processes for their solution in different cases. In this abstract we take only the ordinary Foucault case, to the degree of approximation usually given.

4. Here we neglect terms involving ω^2. Thus we write

$$\ddot{\alpha} = 0,$$

and we write α for α_1, as the difference depends upon the ellipticity of the earth. Also, attending to this, we have

$$\rho = -\frac{r}{a}\alpha + \varpi \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7),$$

where (by (6))

$$S\alpha\varpi = 0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8),$$

and terms of the order ϖ^2 are neglected.

With (7), (5) becomes

$$-\frac{r}{a}V\alpha\ddot{\varpi} = \frac{g}{a}V\alpha\varpi\ ;$$

so that, if we write

$$\frac{g}{r} = n^2\ , \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

we have

$$V\alpha(\ddot{\varpi} + n^2\varpi) = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10).$$

Now, the two vectors

$$\alpha i - a\sin\lambda \quad \text{and } Vi\alpha$$

have, as is easily seen, equal tensors; the first is parallel to the line drawn horizontally *northwards* from the point of suspension, the second horizontally *eastwards*.

Let, therefore,

$$\varpi = x(\alpha i - a\sin\lambda) + y\,Vi\alpha \quad . \quad . \quad . \quad . \quad . \quad (11),$$

which (x and y being very small) is consistent with (6.)

From this we have (employing (2) and (3), and omitting ω^2)

$$\dot{\varpi} = \dot{x}(\alpha i - a\sin\lambda) + \dot{y}\,Vi\alpha - x\omega\sin\lambda\,Vi\alpha - y\omega(\alpha - \alpha i\sin\lambda),$$

$$\ddot{\varpi} = \ddot{x}(\alpha i - a\sin\lambda) + \ddot{y}\,Vi\alpha - 2\dot{x}\omega\sin\lambda\,Vi\alpha - 2\dot{y}\omega(\alpha - \alpha i\sin\lambda).$$

With this (10) becomes

$$V\alpha\left[\ddot{x}(\alpha i - a\sin\lambda) + \ddot{y}\,Vi\alpha - 2\dot{x}\omega\sin\lambda\,Vi\alpha - 2\dot{y}\omega(\alpha - \alpha i\sin\lambda) + n^2x(\alpha i - a\sin\lambda) + n^2y\,Vi\alpha\right] = 0$$

or, if we note that

$$\mathrm{V}.a\,\mathrm{V}ia = a(ai - a\sin\lambda),$$

$$(-\ddot{x} - 2\dot{y}\omega\sin\lambda - n^2x)a\,\mathrm{V}ia + (\ddot{y} - 2\dot{x}\omega\sin\lambda + n^2y)a(ai - a\sin\lambda) = 0.$$

This gives at once

$$\left.\begin{aligned} \ddot{x} + n^2x + 2\omega\,\dot{y}\sin\lambda = 0 \\ \ddot{y} + n^2y - 2\omega\,\dot{x}\sin\lambda = 0 \end{aligned}\right\} \quad \ldots\ldots\ldots \quad (12),$$

which are the equations usually obtained; and of which the solution is as follows:—

If we transform to a set of axes revolving in the horizontal plane at the point of suspension, the direction of motion being from the positive (northward) axis of x to the positive (eastward) axis of y, with angular velocity Ω, so that

$$\left.\begin{aligned} x = \xi\cos\Omega t - \eta\sin\Omega t \\ y = \xi\sin\Omega t + \eta\cos\Omega t \end{aligned}\right\} \quad \ldots\ldots\ldots \quad (13),$$

and omit the terms in Ω^2 and in $\omega\Omega$ (a process justified by the results, *see* equation (15)), we have

$$\left.\begin{aligned} (\ddot{\xi} + n^2\xi)\cos\Omega t - (\ddot{\eta} + n^2\eta)\sin\Omega t - 2\dot{\eta}(\Omega - \omega\sin\lambda) = 0 \\ (\ddot{\xi} + n^2\xi)\sin\Omega t + (\ddot{\eta} + n^2\eta)\cos\Omega t + 2\dot{\xi}(\Omega - \omega\sin\lambda) = 0 \end{aligned}\right\} \quad (14).$$

So that, if we put

$$\Omega = \omega\sin\lambda \quad \ldots\ldots\ldots \quad (15),$$

we have simply

$$\left.\begin{aligned} \ddot{\xi} + n^2\xi = 0 \\ \ddot{\eta} + n^2\eta = 0 \end{aligned}\right\} \quad \ldots\ldots\ldots \quad (16),$$

the usual equations of elliptic motion about a centre of force in the centre of the Ellipse.

5. In the paper this problem is treated with a closer approximation, terms in ω^2, &c., being retained. The conical pendulum—the path of the bob being very nearly a horizontal circle, *i.e.*, the tension of the cord being nearly constant—is next treated; then the case of very great angular velocity, when the path is nearly a circle (in any plane) with centre at the point of suspension. A few sections are devoted to the consideration of the effect of a disturbing body, such as the moon or the sun.

The following Gentlemen were elected Fellows of the Society:—

Isaac Anderson-Henry, Esq.
George Elder, Esq.
Sir Charles A. Hartley, C.E.
David MacGibbon, Esq.
Rev. Thomas Melville Raven, M.A.
Alexander Howe, Esq.
Viscount Walden.
Professor Alexander Dickson.

Monday, 18th January 1869.

Professor KELLAND, Vice-President, in the Chair.

The following Communications were read:—

1. On the Connection between Chemical Constitution and Physiological Action.

On the Physiological Action of the Salts of the Ammonium Bases derived from Atropia and Conia. By Dr A. Crum Brown and Dr Thomas R. Fraser.

Atropia.—Atropia is a nitrile base. All we know of its constitution is, that by the action of strong acids and bases it is decomposed, in accordance with the equation.*

$$\underset{\text{Atropia.}}{C_{17}H_{23}NO_3} + \underset{\text{Water.}}{H_2O} = \underset{\text{Tropic Acid.}}{C_9H_{10}O_3} + \underset{\text{Tropia.}}{C_8H_{15}NO}$$

So that atropia may be considered as tropia, in which one atom of hydrogen has been replaced by tropyl, the radical of tropic acid. Tropic acid belongs to the aromatic series, and is considered by Kraut to be phenylsarcolactic acid—$HO \cdot CH_2 \cdot CH(C_6H_5)COOH$. Of the constitution of tropia we know nothing whatever, except that it is a nitrile base.

Iodide of methyl-atropium.—Iodide of methyl acts very readily on atropia, a good deal of heat is produced, and after the reaction is over, the iodide of methyl-atropium remains as a white mass. From this the excess of iodide of methyl was removed by a current of air, and the dry salt dissolved in water, filtered, and evaporated at a temperature not exceeding 40° C. The concentrated solution thus obtained, on cooling, deposits the salt in prismatic crystals, apparently belonging to the monoclinic system; sometimes part of

* Kraut.—Annalen der Ch. u. Ph., cxxviii. 280, cxxxiii. 87, cxlviii. 238. Lossen.—*Ib.*, cxxxi. 43, cxxxviii. 230.

the salt separates as a heavy oil, which soon crystallises. These crystals have the composition $C_{17}H_{23}NO_3CH_3I$. They are tolerably stable, bearing a temperature of 100° C. without much alteration. When they are powdered, or when their solution is warmed, a pleasant fruity smell is observed.

Sulphate of methyl-atropium.—The sulphate was prepared from the iodide by the method formerly described for the preparation of sulphate of methyl-strychnium, &c. It is a white crystalline salt, very deliquescent, and very soluble in water.

Iodide of ethyl-atropium.—Iodide of ethyl acts readily on atropia, but not so energetically as iodide of methyl. In preparing the iodide of ethyl-atropium, atropia was treated with a considerable excess of iodide of ethyl in sealed tubes at 100° C. for an hour. The remainder of the process, and the preparation of the sulphate, are the same as in the case of the methyl derivative.

The authors intend, on some future occasion, to describe these substances more minutely; for their present purpose, the description given above seems sufficient.

Atropia has a somewhat complicated physiological action, for it directly influences the functions of the cerebro-spinal, and also of the sympathetic nervous system.

The action of the methyl and ethyl derivatives on the cerebro-spinal nervous system is different from that of the natural base, while the action on the sympathetic system is essentially the same.

The principal effects produced by atropia on the cerebro-spinal nervous system are excitation of the spinal cord,* and paralysis of the motor and sensory nerves. In a previous paper, the authors showed that the spinal stimulant action of strychnia, brucia, thebaia, codeia, and morphia, is not possessed by the salts of the ammonium bases derived from these alkaloids, but that in its place these derivatives possess a markedly different paralysing action on the peripheral terminations of motor nerves. They now announce that a similar change occurs in the methyl and ethyl derivatives of atropia. These derivatives are more powerful paralysing substances than atropia itself.

A considerable amount of spinal stimulant and of paralysing action may be produced by a non-fatal dose of atropia; and it is

* See Proced. Roy. Soc. of Ed. vol. vi. 1868-69, p. 434.

probable that the one action is, to a certain extent, antagonistic to the other. As the methyl and ethyl derivatives, however, combine with the ordinary paralysing action of atropia, an additional amount of paralysing action bearing some ratio to the absent spinal stimulant action of the natural base, these derivatives affect the motor nerves much more powerfully than atropia itself. Probably, for these reasons, the salts of methyl- and ethyl-atropium are fatal to the lower animals in much smaller doses than the salts of atropia itself.

Paralysis of the vagi nerves and dilatation of the pupil are caused by these derivatives of atropia.

Conia.—The alkaloid prepared from *Conium maculatum* (hemlock) has been shown by Von Planta and Kekulé* to be a variable mixture of two bases, to which they give the names of "conia" and "methyl-conia." These bases resemble one another very closely in physical properties. Their composition is represented by the formulæ $C_8H_{15}N$ and $C_9H_{17}N$. The chemists above named investigated very completely the action of iodide of ethyl on conia, and proved that conia (or, as it is called in the present paper, *normal conia*) is an imide base, and that methyl-conia is a nitrile base.

The substances examined in the present paper are:—

1*st*, *Conia*—samples of which were obtained from Messrs Duncan and Flockhart, Macfarlan & Co., and Morson. The authors are also indebted to the kindness of Dr Christison for the opportunity of examining the action of a specimen of conia which he prepared in the year 1835.

2*d*, *Methyl-conia*—prepared from hydriodate of methyl-conia, produced by the union of iodide of methyl and *normal conia*.

And 3*d*, *Salts of dimethyl-conium*—obtained by the union of iodide of methyl and the methyl-conia contained in conia as obtained from the plant.

Iodide of methyl acts readily upon conia, producing a syrupy or crystalline substance, which is a mixture of hydriodate of methyl-conia and iodide of dimethyl-conium,—the former produced from the normal conia, and the latter from the methyl-conia. The action of caustic potash decomposes the hydriodate of methyl-conia, setting the base free, while the iodide of dimethyl-conium is unattacked. The two substances can thus be readily separated from each other.

* Annalen der Chemie und Pharmacie, lxxxix. 5.

The authors find that the salts of conia and of methyl-conia very closely resemble each other in action and in poisonous activity. Their action agrees with the descriptions of the effects of conia by the more trustworthy of previous observers. Among the most obvious of the effects on rabbits were stiffness of the limbs, causing difficulty in moving about; spasmodic starts; distinct increase of reflex excitability; gradually increasing paralysis with diminution, and afterwards disappearance, of the increased reflex excitability; and, finally, death by asphyxia. Shortly before death a few starts and feeble convulsions usually occurred, but these symptoms were apparently caused by the advancing asphyxia.

The symptoms in frogs were mainly those of paralysis, and the authors confirm the observations of Kölliker and Guttmann, that this paralysis in the case of ordinary conia is due to a curare-like action. They further find that methyl-conia also acts by paralysing the terminations of motor nerves.

The salts of dimethyl-conia differ from those of conia and of methyl-conia in never directly producing convulsant effects or other symptoms of abnormal activity of the reflex function, and in being much less active as poisons. In rabbits and frogs, the symptoms were invariably those of paralysis; and, in the latter animal, the authors have demonstrated that this paralysis is due to an action on the terminations of the motor nerves.

The samples of conia which have been examined by the authors were found to contain very varying proportions of normal conia and of methyl-conia; but as these two substances appear to be about equally active as poisons, it is probable that the very variable potency of commercial conia is due to its adulteration with a greater or less amount of water, and, possibly, also to the presence of varying quantities of ammonia.*

* When the authors had nearly concluded their investigation on conia, they received a communication from MM. Jolyet and Cahours of Paris, informing them that these physiologists were ready to publish a paper upon the relative action of the salts of conia, ethyl-conia, and diethyl-conium. In order to secure simultaneous publication, it was arranged that the two papers should be communicated on the same day—the one to the Academy of Sciences of Paris, and the other to this Society.

2. Note on the Determination of Heights, chiefly in the Interior of Continents, from Observations of Atmospheric Pressure. By Alexander Buchan, M.A., Secretary of the Scottish Meteorological Society.

The weight or pressure of the atmosphere is ascertained by the mercurial barometer, the aneroid, or from the temperature of the boiling point of water. The height of a hill is measured barometrically, from observations made simultaneously at its base and top, and the application of certain well-known formulæ. The height of a place at no great distance from another place whose height is known, and at which observations are made about the same time, may similarly be ascertained with a close approximation to the truth.

But, with regard to places far from any place of known elevation, or from any place at which meteorological observations are made, it is plain that the height can only be computed by assuming a certain pressure as the sea-level pressure at that place.

In the Table giving the reductions of heights from Captain Speke's observations, it is stated (Journal of the Royal Geographical Society, vol. xxxiii.) that a mean pressure of 29·92 inches was assumed as the mean sea-level pressure,—that is, if those parts of Africa visited by Speke had been on the same level with the sea, it is assumed that the mean pressure of the atmosphere would have been 29·92 inches.

In the last revised "Hints to Travellers," prepared by the Royal Geographical Society, and published in the Journal, vol. xxxiv., it is stated at page 286, "When the boiling point at the upper station alone is observed, we may assume 30·00 inches, or a little less, as the average height of the barometer at the level of the sea. The altitude of the upper station is then at once approximately obtained from the tables." So far as I have been able to ascertain, this mean height of the barometer has been generally accepted by travellers as applicable to all seasons, and to all parts of the globe at great distances from Meteorological Observatories. Unfortunately, it has hitherto been generally the practice for travellers, or those who have been intrusted with reducing their observations, to give only the heights deduced from the observations, with a curious minute-

ness of accuracy, and not the observations themselves. Since the tables which have been prepared for travellers are calculated on the assumption that 29·92 inches, or 30·0 inches, is the zero point for heights, there can be little doubt that, by this method, the heights of many plateaux and mountains of the globe have thus been determined.

From my paper, read before this Society in March 1868, on the Mean Pressure of the Atmosphere over the Globe, illustrated with three charts, showing the *Mean Isobaric Curves* for July, January, and the year, it may be seen that a pressure of from 29·9 to 30·0 inches is very near the mean annual pressure over the greater part of the globe, particularly over those portions of it explored by travellers. But when we examine the months, it is at once apparent that 29·9 inches is very far from the mean pressure in many regions. This point will be illustrated by the pressures at Barnaul, Siberia, which on an average of 19 years are, reduced to 32° and sea-level, as follows:—

Mean atmospheric pressure at Barnaul in	July,	29·536 inches.
" " "	January,	30·293 "
" " "	Year,	29·954 "

Suppose, now, it be proposed to ascertain the height of Lake Balkash on some day in July, the pressure at the time being the average of the month. Let the observed pressure be 28·8 inches reduced to 32° F., and the temperature of the air be 70°·0, then if the sea-level pressure be assumed to be 29·9 inches, it is plain that the difference due to height is 1·10 inches; in other words, the height of the lake would be, in round numbers, 1080 feet. But since the sea-level pressure of this locality, which is nearly that of Barnaul, is 29·536 inches, the difference of pressure due to height is only 0·736 inch; the height, therefore, is only about 730 feet. Again, if in January, when the barometer is the mean of the month, the pressure at Lake Balkash was observed to be 29·42 inches, and the temperature of the air 1°·0, assuming that 29·9 inches is the mean sea-level pressure of January, 0·48 inch is the difference of pressure due to height—that is, the lake is about 400 feet above the sea. But since the mean pressure is nearly 30·3 inches, 0·88 inch is the pressure due to height; the lake is therefore nearly 730 feet above the sea. Thus in July the lake would be made 350 feet too high, and in January 330 feet too low—the

difference of the two observations, each being here supposed to be taken under the most favourable circumstances, and with the greatest accuracy, being 680 feet.* Observations made in the first half of April, or in the latter half of October, when the pressure is the mean of the year, supply the best data for the calculation of heights.

If the best physical atlases be examined, and the heights, given by different authorities, of table-lands and mountains, of Central Asia, Central Africa, and the highlands of the United States and British America be compared, considerable confusion will be found to prevail.

One or two examples may be given to show the application of all this. From barometric observations made on the 28th November 1838, the level of the Dead Sea below that of the Mediterranean was calculated to be 1429 feet. The real depth of this sea below the level of the Mediterranean, as determined by the English engineers by levelling, is 1296 feet. Now, since the mean pressure of the atmosphere over the region of the Dead Sea in the end of November is about 30·035 inches, it is seen, if the sea-level pressure was assumed to be 29·9 inches, how the lake came to be lowered 133 feet.

Much interest is at present attached to the heights of Central Africa. The following mean pressures at 32° and sea-level bear on this interesting question:—

	January. inches.	July. inches.		January. inches.	July. inches.
Malta	30·07	30·01	Cape Town	29·97	30·20
Algiers	30·15	30·06	Graff Reinet	29·91	30·22
Laghouat (Algeria)	30·07	29·86	Maritzburg	29·89	30·19
Gibraltar	30·18	30·06	Mauritius	29·95	30·19
Christiansborg	29·92	30·04	Aden	30·03	29·69
St Helena	30·05	30·18	Alexandria	30·06	29·80
Grahamstown	29·91	30·15			

Thus the difference at Graff Reinet and Maritzburg between the January and July pressures amounts to about 0·30 inches. From this it may be inferred that, in calculating heights along the Zambesi, from observations made at different seasons, if no allowance be made for the monthly variation, but if 29·92 inches be assumed as

* The height of Lake Balkash, according to the Russian explorers Ssemonoff and Golubeff, may be anywhere between 530 feet as given by the former, and 1200 feet as given by the latter. For a large number of heights made use of in writing this note, the author is indebted to Mr Keith Johnston, jun.

the height for all seasons, the results from observations made in January will differ from 250 to 300 feet from those obtained from observations made in July at the same place. If no account be taken of the daily variation of the pressure, the observations made in July at 9 A.M. will give a difference of from 350 to 400 feet in height as compared with results from observations made in July at 4 P.M. All this large error is avoided when the monthly and the daily variations are allowed for.

It has been seen that the summer pressure in Central Asia falls in July to about 29·500 inches. It might be inferred by analogy that the pressure in Central Africa also falls considerably below 29·92 inches over those regions where the sun is nearly vertical; and, as a consequence, that this space of low pressure moves north and south with the sun, attaining its northern limit in July, and its southern in January. The figures in the table given above fully bear out this supposition. Thus, in July at Algiers the mean pressure is 30·06; but at Laghouat, between 280 and 300 miles inland, the pressure is only about 29·86 inches; at Alexandria it is 29·80; and at Aden, only 29·69 inches; and since, in the same month, according to Speke, the wind in Central Africa near the equator and long. 32° 20′ E. is almost constantly S.E., it is probable that the pressure there is lower than at Aden. Taking the whole facts into consideration, it can scarcely be less than 29·70 inches, though probably it is lower. Again, in January the pressure at Cape Town being 29·97 inches, at Graff Reinet 29·91 inches, and at Maritzburg 29·89 inches, points still further to a diminution of pressure in the centre of southern Africa at this season, increasing from the coast—falling, probably to between 29·70 and 29·80 inches. Hence, if we assume 29·70 inches as the low pressure which accompanies the sun over those parts of Africa where he is nearly vertical, we shall not be far from the truth.

Let us apply this reasoning to the determination of the height of Albert Nyanza from Sir Samuel W. Baker's observation of the boiling point of water. The observation was made in lat. 1° 14′ N., long. 30° 50′ E, on 14th March 1864, between 8 and 10 A.M., probably at 9 A.M. The boiling point of the thermometer was 207°·8, but as it changed while in Sir Samuel Baker's possession, it is supposed that the true reading was about 207°·3, which corresponds

to a pressure of 27·231 inches.* But since the observation was made about 9 A.M., when the pressure is about the maximum of the day, subtracting ·043 inch as the correction for daily range in July, we obtain as the mean pressure of the day 27·188 inches. If we assume the sea-level pressure to be 29·70 inches, the difference due to difference of height will be 2·512 inches, and the temperature of the air being at the time 84·0, the height of Albert Nyanza will be in round numbers about 2550 feet, or considerably under the height usually given.

Similarly, by the same reasoning, Gondokoro, calculated from Sir Samuel W. Baker's observations to be 1999 feet in height, will be only about 1800 feet above the level of the sea.

Considering the small difference within the tropics in the mean pressure of any month, say July, from year to year, it follows that if recent African travellers had been provided with good thermometers for determining the boiling-point of water, and had made carefully conducted observations with them, noting the precise hour and month of the observations, one of the great problems of African travel would have been already solved, viz., whether Lake Tanganyika does or does not flow into Albert Nyanza, unless the difference of level between these two lakes is comparatively small. But since travellers have been given to understand that the heights deduced from their observations may be in error to the extent of from 300 to 500 feet, less care has been bestowed in making such observations than would otherwise have been the case.

In extra-tropical regions the height of the barometer is much more fluctuating, and the pressure during any month from year to year varies more than within the tropics. But even in these regions the limits of error are much less than are usually supposed, if care be taken to make the observations full and precise, so that when they come to be reduced it may be in the power of the meteorologist to value them at their proper worth. This remark may require a little explanation.

In temperate regions barometric fluctuations are more frequent and of greater amplitude in such countries as Great Britain, which are situated between a continent on the one hand and an ocean on

* Regnault's Tables, revised by Moritz.

the other, than in the interior of continents in the same latitudes. Now, since it is to the interior of continents, viz., Asia, Africa, North and South America, and Australia, that these remarks on the discussing of heights are intended to apply, the limits of error of single observations, or groups of observations, of the pressure of the atmosphere, are much less than one accustomed to observe barometric fluctuations in Great Britain might be led to suppose. Hence, if the mean monthly sea-level pressure of the part of the earth's surface where the observation is made be kept in mind, the difference between this pressure and the observed pressure will be a tolerable approximation to the true difference of pressure due to the elevation of the place.

But a still closer approximation may be reached. All examination of weather on a large scale shows, in the most conclusive manner, that barometric fluctuations are always attended with changes of weather of a well-marked and determinate character. Hence, conversely, if travellers kept a careful record of the weather some time before and some time after they made their observations of the pressure of the atmosphere, some idea could be formed as to whether the observed pressure was above or below the mean pressure of the season at the place.

Thus, suppose that for some time before and after the observation the weather was fine and of a steady character throughout, the nights not much colder and the days not much hotter than usual, the winds light, or if moderate, continuing in one direction, and the state of the sky with respect to cloud much the same from day to day, it might be assumed that the pressure was the average of the season. Observations carefully made under these conditions are entitled to be ranked in the first class, as being the most trustworthy that can be obtained.

But if the nights have been for a day or two colder, the days hotter (in the sun), the air drier, and the winds lighter, and calm weather more prevalent than usual, then it is probable that the pressure at the time of observation was above the average of the season.

Again, suppose, in the north temperate zone, the air to have become warmer and moister, the sky clouded, rain to have fallen, and the wind veered from E. or S.E., by S. and S.W. to N.W., or

suddenly shifted to W. or N.W., and the weather then to have become colder and clearer and the air drier, it is certain that a storm of greater or less magnitude has passed over the region, and since such storms are attended with great fluctuations of the barometer, it is plain that if the observation of pressure was made during these changes, it is worse than useless as a datum for the determination of the height of the place. It should, therefore, be altogether rejected.

These cases are given as examples of the method by which observations, as made by travellers, should be critically examined before they are made use of in calculating heights. It is probably from inattention to these simple directions—travellers not recording the required data, which can all be recorded without instruments, and computers not giving weight to such observations when recorded—that a large number of the grosser discrepancies, given in works of Physical Geography, have arisen. Many of the larger errors are, of course, due to the use of imperfect instruments and a want of practice in the observer.

An illustration of errors in the statement of heights may be given. The following places are situated in the neighbourhood of the Ural Mountains; the heights are those given by the most recent authorities, and a column is given showing the number of years for which the averages of mean annual pressure have been calculated:—

Place.	Lat. N.	Long. E.	Height in Feet.	Years of Average.	Mean Pressure at 32° and sea-level
	° ′	° ′			Inches.
Bogoslovsk, . .	59 45	60 2	600	26	29·862
Nijni-Tagilsk, .	57 57	59 53	730	21	30·088
Catherinenburg, .	66 49	60 35	800	18	29·835
Zlatoust, . . .	55 10	59 40	1200	28	29·835

From the above annual mean pressures it is evident that the height of Nijni-Tagilsk is over stated, the true height being, probably about 250 feet less than what has hitherto been assigned to it.

Since it has, unfortunately, been the general practice not to publish the original observations, but only the heights deduced from them, it will be impossible, except in a comparatively small number

of instances, to apply the principle brought forward in this paper to past observations.

Observations for the ascertaining of heights must, to be satisfactory, include the following particulars:—

1. Latitude and longitude of the place.
2. The date of the observation, giving exactly the year, the month, the day of the month, and the hour of the day.
3. The observation itself exactly as made; if with a barometer or aneroid, the pressure to be given; if with a thermometer, the boiling-point to be given, and not merely its equivalent in pressure.
4. The temperature of the air in shade.
5. The weather for two days before and after the observation, showing the temperature of the air, its probable humidity as made known by the feelings or by its effects on surrounding objects, the amount of cloud, the rainfall, the direction, veerings, shifts, and force of the wind, together with any striking phenomena that may occur.

To these might be added, if possible, observations of the wet-bulb thermometer.

It will be evident from these remarks that the Physical Geographer will require the practised Meteorologist to aid him in settling the important physical problem of heights for large portions of the earth's surface.

3. Notice of a Remarkable Mirage Observed in the Firth of Forth. By Dr Christison.

Some years ago, when visiting on the 31st of May the beautiful scenery of Dalmeny Park, I observed about three in the afternoon, from the terrace on which stand the ruins of Barenbougle Castle, some remarkable examples of mirage on the Firth of Forth, which are perhaps worthy of record.

The atmosphere was uncommonly clear in every direction, sunshiny, warm, calm; and what little wind there was came from the south-west. But a black, sultry-looking cloud was forming at the same time in the north-west, from which in an hour and a half afterwards, as I was on my way home to town, a severe thunder-

storm gradually spread south-eastward over the whole Firth and surrounding country.

Looking eastward towards the mouth of the Firth, while the weather continued very fine, I was surprised to observe that the northern edge of the cone of North Berwick Law, twenty-four miles distant, at about one-third of its slope from the summit, suddenly ceased, and had given place to a sheer perpendicular precipice, overhanging its base. At the same time the spit of land of East Lothian, which is usually seen to extend more than a mile from the Law towards the Fife coast, had disappeared; the ocean had taken its place, and the overhanging precipice of the hill seemed to dip into the water. I carefully examined these appearances with a Ross's telescope of very sharp definition, and could easily ascertain that there was no mist anywhere; and the apparent precipice and apparent sea presented no character different from those of the true sky-line of the hill above, or the true sea northward from where Gullane Point terminates the land as seen from my station.

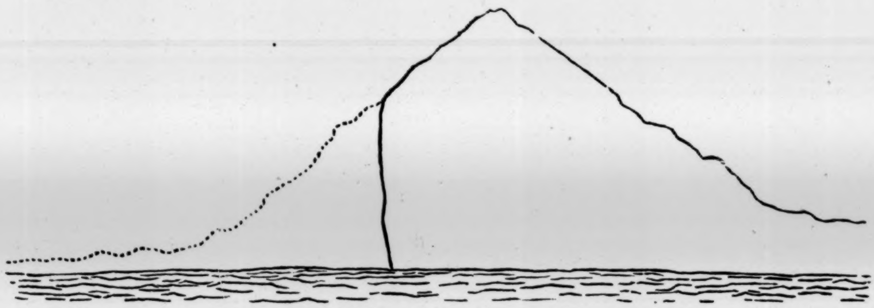


Fig. 1.—North Berwick Law, twenty-four miles.

I then also noticed that what appeared to be the south point of Inchkeith as seen from Barnbougle, eight miles distant, had thrown out southward a line of three detached rocks, with sea separating them from one another, and likewise with sea apparently passing under them, as if they were suspended in the air a few yards above the water; and that in like manner the water seemed, as it were, to have passed to some distance under the point of the island itself, raising a long low line of dark land a little into the air. It was impossible to put an end to this illusion with the telescope; on the contrary, it was rendered more distinct and apparently real.

But the most remarkable transformation was effected in a sloop which was sailing obliquely across my line of vision from south-west to north-east, apparently at the Granton Ferry, about four miles off. From the set of its sails, and the darkness of a great extent of the Firth around, the vessel was evidently carrying with it a brisk favouring breeze from the south-west; and during the fifteen or twenty minutes that my observation lasted it must have changed its place by two miles. But during all that time it presented the appearance of a narrow gap in its entire hull immediately before its single mast, and a very much wider one behind the mast. The sea was visible through both gaps; and with the telescope it was easy to observe the helmsman at the tiller on a poop completely and widely separated from the forward part of the vessel. The ferry steamer appearing from Granton on her passage to Burnt-

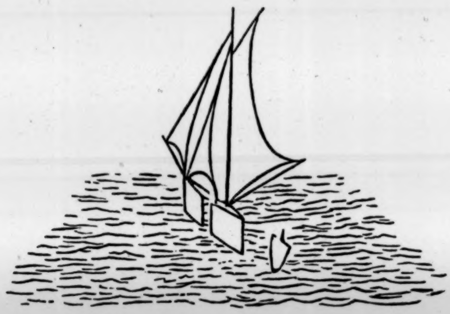


Fig. 2.—Vessel six miles off.

island, I was anxious to see what might happen to her when she arrived in the same water with the sloop. But when she came into a line with the sloop, it was at once apparent that the sloop was about two miles farther off; and she had previously appeared to be nearer, in consequence of the sheet of dark broken water in which she had been sailing being apparently raised above the level of the calm white water surrounding it on all sides over the rest of the Firth.

During a period of about twenty minutes, spent in watching these alterations of form in North Berwick Law, Inchkeith, and the sailing vessel, I could not detect the slightest change in the character of the appearances.

I am unwilling to attempt an explanation of these singular

deviations from optical rectitude, because I do not see how recognised theory will explain them all. But I may venture to suggest that such instances of mirage in our Firth and elsewhere may deserve attention as possible prognostics of weather. On the occasion in question there were evident signs of a widely extended disturbance of the usual condition of the atmosphere; and the occurrence of a violent thunderstorm afterwards, which lasted far into the evening, is not unworthy of notice.

Often as I have been at Barnbougle Castle in beautiful summer afternoons, and often as I have been on the shores of the Firth in all parts, as well as on its waters, I never happened to notice any mirage before. Subsequently, however,—I do not recollect from what position, but I rather think from Leith Pier,—I observed that Aberlady Bay seemed to lead to a strait with perpendicular cliffs on either side, the promontory on which Gosford, Dirleton, Gullane, and North Berwick stand, being converted into a beautiful and extensive island; and the illusion was rendered complete by a vessel under full sail bearing to all appearance directly for the mouth of the imaginary strait.

My son also informs me that on another occasion he observed with the naked eye, but still better with the aid of a telescope, from Leith Pier, the same promontory cut into eight or ten islands, apparently reflected in an apparently calm sea, which joined by a well-defined margin the real sea itself, much disturbed at the time by a brisk breeze. The appearance of these islands, as seen through the telescope, was exceedingly beautiful, as they seemed to be clothed with fine trees, and, reflected in the calm ocean, were suggestive rather of a tropical than a northern scene.

It is probable that such appearances are not infrequent—so frequent, at least, as to give adequate opportunities for studying them; and that we do not hear of them merely from inobservance; for my son adds that, although many people, sailors as well as others, were walking on the pier at the time, not one seemed to take notice of the remarkable phenomena, which strongly attracted his attention and that of his brother who accompanied him.

The following gentlemen were elected Fellows of the Society:—

William C. M'Intosh, M.D.
Henry Marshall, M.D., Clifton.
William Rutherford, M.D.

Monday, 1st February, 1869.

The Hon. LORD NEAVES, Vice-President, in the Chair.

The following Minute of Council was read:—

"The Council of the Royal Society desire to record in their minutes the grief which they have felt in the death of James D. Forbes, Esq., lately Principal of St Andrews University, and their sense of the loss thus sustained—of one who was so great an ornament to science, and so long and so intimately connected with this Society. As a scientific inquirer, and as an academical instructor, the name of Dr Forbes will be held in reverence by all who knew him, or who benefited by his exertions; and it is a subject of deep regret that he should be cut off in the prime of life, and the scientific world deprived of his services at a time when his appointment as Principal of St Andrews University had placed him in a position where, if life and health had allowed, he might, by his further labours, have added to his own reputation and to the range of scientific discovery. The Council cannot remember the able, assiduous, and conscientious services rendered to the Royal Society, when he held the appointment of their Secretary, without feeling how deep a debt of gratitude the Society owes him. The Council direct that a copy of this minute be transmitted to Mrs Forbes, with a suitable expression of their sympathy with her in her great affliction. The Council trust that Mrs Forbes will be sustained under her severe bereavement, and they are sure that if public and individual sympathy can assist in alleviating grief, that consolation will be afforded in no ordinary degree."

The Secretary announced the receipt of letters from Professors Kirchhoff and Virchow, thanking the Society for their election as Foreign Honorary Fellows.

The following Communications were read :—

1. Practical Note on Intensified Gravity in Centrifugal Governors. By Professor C. Piazzi Smyth.

This paper, after a short introduction upon previous publications and experiences, describes in a plain and practical manner four steps of improvement recently made in the centrifugal governor of an equatorial driving clock; and touches on

1. The number of the pendulums;
2. Their weight and momentum;
3. Their chronometric principle; and
4. The intensification of the effect of gravity upon them, chiefly to increase the promptitude and energy of the centrifugal action.

This last improvement the author regards as the most important to draw attention to, because it is capable of imparting extreme quickness and sensibility to the actions of all kinds of centrifugal governors; and although not absolutely new, it does not seem yet to be sufficiently known or employed in many practical cases, where it might be of the utmost use, as in preventing some classes of disasters which are in these days happening far too frequently to sea-going screw-steamers.

The paper has since been printed in full in the "Practical Mechanic's Journal" for March 1, 1869.

2. Mr Mill's Theory of Geometrical Reasoning Mathematically Tested. By W. R. Smith, Esq., Assistant to the Professor of Natural Philosophy. Communicated by Professor Tait.

An amusing and instructive example of the way in which logicians are accustomed to dogmatise upon the theory of sciences that they do not understand, is afforded by Mr Mill's explanation of the nature of geometrical reasoning.

Those who remember that Mr Mill assures Dr Whewell that he has conscientiously studied geometry (Logic, 7th ed. I. 270), will probably find some difficulty in believing that the demonstration of Euc. I^5, which Mr Mill offers as an illustration of the justice of his

theory of geometrical reasoning, depends on the axiom, that triangles, having two sides equal each to each, are equal in all respects. Such, nevertheless, is the case; and when one sees this absurdity pass unmodified from edition to edition of Mr Mill's Logic, and when even Mansel, Mr Mill's watchful enemy, tells us that "against the form of the geometrical syllogism, as exhibited by Mr Mill, the logician will have no objections to allege" (Mansel's Aldrich, 3d ed., p. 255), one cannot but think that logic would make more progress if logicians would give a little more attention to the processes they profess to explain.

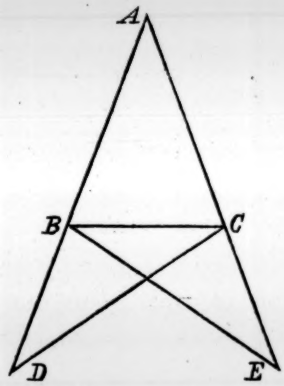
It may perhaps be worth while to show how Mr Mill was led into this extraordinary mistake. We shall find that Mr Mill chooses rather to sacrifice geometry to his philosophy, than to modify his philosophy in accordance with the facts of geometry.

Mr Mill holds that all general knowledge is derived from experience; meaning by experience the comparison of at least two distinct experimental facts. In other words, all knowledge is ultimately gained by induction from a *series* of observed facts. That any general truth can be got at intuitively, by merely looking at one case, Mr Mill emphatically denies. The fact that two straight lines cannot enclose a space, is not self-evident as soon as we know what straight lines are [*i.e.*, can mentally construct such lines]; but is got at only by experiments on "real" or "imaginary" lines (Logic, I. pp. 259, 262). Now it is certain, that in the demonstrations of Euclid, we are satisfied of the truth of the general proposition enunciated, as soon as we have read the proof for the special figure laid down. There is no need for an induction from the comparison of several figures. Since then one figure is as good as half a dozen, Mr Mill is forced to the conclusion that the figure is no essential part of the proof, or that "by dropping the use of diagrams, and substituting, in the demonstrations, general phrases for the letters of the alphabet, we might prove the general theorem directly" from "the axioms and definitions in their general form" (p. 213).

We may just mention, in passing, that this view, combined with the doctrine that the definitions of geometry are purely hypothetical, leads Mr Mill to the curious opinion that we might make any number of imaginary sciences as complicated as geometry, by applying real axioms to imaginary definitions. We mention this

merely to illustrate Mr Mill's position—our present business is to see how these views of geometry work in practice.

Mr Mill's example, as we have said, is Euclid I[5], which he undertakes to deduce from the original deductive foundations. We have first (p. 241) some preliminary remarks, which afford a remarkably happy instance of the way in which Mr Mill is accustomed to keep himself safe from all opponents, by alternately supporting each of two contrary views of a subject. "First," says he, speaking of the angles ABE, CBE; ACD, BCD, "it could be perceived intuitively that their differences were the angles at the base." If this intuition is really a step in the proof, then, since intuition is just actual looking at the figure, what becomes of the doctrine that the figure is not essential, or of the still more fundamental doctrine, that no general truth can flow from a single intuition?* In this, however, Mr Mill only falls foul of himself. A more serious matter is, that when he sets about his regular demonstration, he falls foul of the truths of geometry.



Having shown that AD=AE, Mr Mill proceeds thus:—"Both these pairs of straight lines" [AC, AB: AD, AE] "have the property of equality; which is a mark that, if applied to each other they will coincide. Coinciding altogether, means coinciding in every part, and of course at their extremities, D, E, and B, C." Now, "straight lines, having their extremities coincident, coincide. BE and CD have been brought within this formula by the preceding induction; they will, therefore, coincide." [!] If Mr Mill generalises this conclusion, I think he will find it to be that two triangles, having two sides of each equal, are equal in all respects; and from this theorem he may at once conclude, by his own fourth formula ["angles having their sides coincident, coincide"], that

* This is no mere slip on Mr Mill's part. To show that the angles at the base are the differences of the angles in question, without appealing to the figure, we must have a new axiom [proved, of course, by induction !] viz., that if a side of a triangle be produced to any point, the line joining that point with the opposite angle falls wholly without the triangle.

angles contained by equal straight lines are equal! It is clear that Mr Mill did not see that the point is to show that, *the triangles ABE, ACD, remaining rigid*, AB may be applied to AC, and AE to AD at the same time. But this can only be brought out by figuring to oneself AB moved round to coincide with AC, and then the triangle ABE rotated about AB through two right angles; and this process was not competent to Mr Mill, whose theory bound him to prove the equality of the triangles by pure syllogism from the two formulas, "equal straight lines, being applied to one another, coincide," and "straight lines, having their extremities coincident, coincide."

But Mr Mill may say, "I have only to add, that equal angles applied to one another coincide."

Very well, you have then three syllogisms:—

Equal straight lines coincide if applied;	Equal straight lines coincide if applied;
AC, AB, are equal.	AD, AE, are equal.

Equal angles coincide if applied;
CAD, BAE, are equal.

Logically these three syllogisms can give only three independent conclusions:—

AC, AB coincide if applied.	AD, AE coincide if applied.

The angles CAD, BAE coincide if applied:—

but by no means the ONE conclusion that the rigid figures ABE, ACD coincide if applied. If Mr Mill still contends that there is no need for intuition here, let him substitute for the words "equal straight lines," "equal arcs of great circles." The premises of his syllogisms are still all right; but, owing to circumstances that must be *seen* to be understood, the spherical triangles cannot be made to coincide.

There are only two courses open to Mr Mill—either to confess that the attempt to square geometry with a preconceived theory has forced him into a grossly erroneous demonstration, or to invent a new formula—viz., that if in two plane figures any number of consecutive sides and angles taken one by one may be made to coincide, they may also be made to coincide as rigidly connected wholes. But, then, Mr Mill must maintain that the man who reads Euc. I[4] for

the first time does not at once conclude the general truth of this formula from the one figure before him, but either brings the formula with him to the proof as a result of previous induction, or requires to pause in the proof, and satisfy himself of the truth of the formula by a comparison of a series of figures.

It is easily shown, by the same species of analysis as we have adopted here, that wherever a real step is made in geometry we must either use the figure or introduce a new general axiom [not of course in mere converses, as Euc. I^{19}, I^{25}]. All geometrical construction is in the last resort a means of making clear to the eye complicated relations of figures.

Now, if we can at once and with certainty conclude from the one case figured in the diagram to the general case—if, that is, axioms are proved not by induction, but by intuition, and are necessarily true—there is no difficulty about geometrical reasoning; but if each new axiom is gained by a new induction (and that on Mr Mill's showing an "*inductio per enumerationem simplicem*,") we get a difficulty which Mr Mill curiously enables us to state in his own words (I. p. 301)—"If it were necessary," in adding a second step to an argument, "to assume some other axiom, the argument would no doubt be weakened." But, says Mr Mill, it is the same axiom which is repeated at each step. If this were not so, "the deductions of pure mathematics could hardly fail to be among the most uncertain of argumentative processes, since they are the longest." If now we do call in new axioms whenever we construct an essentially new figure, must not Mr Mill admit, on his own showing, that every advance in geometry involves an advance in uncertainty; that the geometry of the circle is less certain than that of the straight line, solid geometry than plane, conic sections than Euclid, &c.? Surely this is a *reductio ad absurdum* of the whole theory.

The principles of geometry involved in the question are so important that we may profitably separate them from Mr Mill's blunders in a special case.

I. The proofs of geometry are clearly not inductive. There is no mental comparison of various figures needed during the proof. The inductions involved (if any), must have been previously formed.

II. The proofs then must be reduced either to actual perception

(intuition), or to deduction from axioms. But since the proof is general, the former assumption involves the reality of general intuition, *i.e.*, of a general judgment from a single perception.

III. The theory of intuition is sufficient, but is disputed in two interests:—

(α) In the interest of syllogism, which claims to give indefinitely extensive conclusions from limited premises [but many, as Whewell, hold that these premises are intuitive axioms].

(β) In the interests of empiricism, which makes all arguments be ultimately from particulars to particulars.

Mr Mill combines the two objections.

Now we have seen that if objection (α) falls (*i.e.*, if the premises of geometry are not reducible to a limited number of axioms from which everything follows analytically), the security of geometric reasoning can be established only if each premise has apodictic certainty. To overthrow Mill's whole theory, it is therefore enough to show the fallacy of the limited-number-of-axioms hypothesis. On this we observe:—

1*st*, The axioms are more numerous than Mr Mill thinks, for his proof of Euc. I^5 is lost for want of more axioms.

2*d*, The indefinite extension of geometry depends on the power of indefinitely extended construction [but where there is construction there is intuition—nay, mental intuition is mental construction]. Now here our opponents may suppose [A] that the general conclusion really flows from the particular construction, which, in the language of logic, supplies the middle term. But since the construction is particular, we should thus be involved in the fallacy of the undistributed middle. Again, [B] it may be said that the construction is only the sensible representation of a general axiom. But as the construction is new and indispensable, the general axiom must be so also. Therefore, if geometry is proved from axioms, these axioms must be unlimited in number.

3*d*, Obviously it is not by logic that we can satisfactorily determine how far geometry contains synthetic elements peculiar to itself. We have, however, in analytical geometry a ready criterion how far geometry can be developed without the addition of new geometrical considerations.

Now we find that we cannot begin analytical geometry from the

mere axioms and definitions. We must by synthetic geometry, by actual seeing, learn the qualities of lines and angles before we can begin to use analysis. Then, given so many synthetic propositions, we can deduce others by algebra; but only by a use of actual intuition, *first*, in translating the geometrical enunciation into algebraic formulæ; and, *second*, in translating the algebraic result (if that result is not merely quantitative) into its geometric meaning. The answer to a proposition in analytical geometry is simply a rule to guide us in actually constructing, by a new use of our eyes or imagination, the new lines which we must have to interpret the result. Analysis does not enable us to dispense with synthetic constructions, but simply serves to guide us in these constructions, and so to dispense more or less completely with the tact required to find out the geometrical solution. This is true in every case, but most obviously in the investigation of new curves. The tracing of curves, from their equations, is a process in which no man can succeed by mere rule without the use of his eyes. Suppose asymptotes, cusps, concavity, everything else found, the union of these features in *one* curve will remain a synthetic process.

Still more remarkable is the use made in analysis of imaginary quantities. To the logician an imaginary quantity is nonsense, but geometrically it has a real interpretation. The geometrical power gained bya new method like quaternions, is radically distinct from that gained by the solution of a new differential equation. The latter is a triumph of algebra, the former is a triumph of synthetic geometry—the discovery of a whole class of new guides to construction.

Professor Tait remarked that an excellent and interesting instance of the incapacity of metaphysicians to understand even the most elementary mathematical demonstrations, had been of late revived under the auspices of Dr J. H. Stirling. His name, with those of Berkeley and Hegel, formed a sufficient warrant for calling attention to the point.

It is where Newton, seeking the fluxion of a product, as ab, writes it in a form equivalent to

$$\frac{1}{dt}\left[(a + \tfrac{1}{2}\dot{a}dt)(b + \tfrac{1}{2}\dot{b}dt) - (a - \tfrac{1}{2}\dot{a}dt)(b - \tfrac{1}{2}\dot{b}dt)\right]$$

which gives, at once, the correct value

$$a\dot{b} + b\dot{a} .$$

Now Berkeley, Hegel, Stirling, and others, have all in turn censured this process as a mere trick (or in terms somewhat similar) and say, in effect, that it is essentially erroneous. The fact, however, is that, as in far greater matters, Newton here shows his profound knowledge of the question in hand; and adopts, without any parade, a method which gives the result *true to the second order of small quantities.* The metaphysicians cannot see this, and Dr Stirling speaks with enthusiastic admiration of the clear sightedness and profundity of Hegel in detecting this blunder, and for it "harpooning" Newton!

What Newton seeks is the rate of increase of a quantity at a particular instant. Instead of measuring it by the rate of increase *after* that instant (as the metaphysicians would require) he measures it by observing, as it were, for equal intervals of time *before and after* the instant in question.

Any one who is not a metaphysician can see at once the superior accuracy of Newton's method, by applying both methods to the case of a rapidly varying velocity; such as that of a falling stone, or of a railway train near a station.

In reference to what Professor Tait had said, Mr Sang remarked, that the line of argument attributed to Newton had been used by John Nepair before Newton's birth. Nepair's definition of a logarithm runs thus (Descriptio, lib. i. cap. i. def. 6), (Constructio, 23, 25) that if two points move synchronously along two lines, the one with a uniform velocity (arithmeticè), the other (geometricè) with a velocity proportional to its distance from a fixed point, the distance passed over by the first point is the logarithm of the distance of the second from the fixed point. In order to compare this variable velocity at any instant with the constant velocity, he takes a small interval of time preceding, and another succeeding the given instant, shows that the true velocity is included between the two velocities thus obtained, and (28, 31) takes the arithmetical mean as better than either, and as true (*inter terminos*).

It may be added, that Nepair devotes several sections of his

Constructio to the discussion of the doctrine of limits (*de accuratione*); that his logarithms were denounced by the metaphysicians of his day as founded on the false system of approximation, but that, fortunately for the progress of exact science, their objections were unheeded.

Sir W. Thomson said, that the metaphysicians, wishing to find the speed of a vessel at 12 o'clock from an hour's run, would choose the hour from 12 to 1; whereas Napier, Newton, and the rest of the world, would take it from 11·30 to 12·30.

3. Note on Captain A. Moncrieff's system of working Artillery. By R. W. Thomson, Esq., C.E. A Model of Captain Moncrieff's Gun Carriage was exhibited, and its mode of action was shown.

4. Note on an undescribed variety of Flexible Sandstone. By T. C. Archer, Esq.

The following gentleman was admitted a Fellow of the Society:—

Dr R. Craig Maclagan, F.R.C.P.E.